THE THEORETICAL DISTINCTION BETWEEN MULTIPLE ALLELOMORPHS AND CLOSE LINKAGE

PROFESSOR CASTLE'S difficulty in understanding the distinction made by Mr. Dexter[1] is owing to his unfamiliarity at first hand with the phenomenon of linkage. The distinction between allelomorphs and close linkage has already been given several times elsewhere and need not be repeated; but if Professor Castle has failed to note it, or to see its significance, it is probable that others may have done the same. I may be pardoned, therefore, for attempting once more to show why, for clear thinking, it is important to keep in mind the difference between allelomorphs and close linkage. Furthermore, since we have here one of the newest developments of Mendelism, it seems to me that it may be worth while not to let Professor Castle's criticism pass unchallenged.

Dexter pointed out that the mode of treatment that Nabours followed in the analysis of his results is the procedure of multiple allelomorphism, although Nabours does not seem entirely conversant with the fact, but treats the results as though they were regular phenomena. In one case, however, Nabours got an unconformable individual. Dexter points out that if this case is not due to non-disjunction (a known process that will cover such cases) it shows that here at least the factors involved are not allelomorphs, but must be treated as though closely linked.

How could the matter be put more directly? I confess I am at somewhat of a loss to discover why Professor Castle is confused. Perhaps it is the subsequent development of Dexter's explanation that has troubled him. Let us again try to make the distinction clear.

If the factors B and E are not allelomorphic to each other then each must have another allelomorph. This is nothing but pure Mendelism, which no one will, I suppose, dispute. It is entirely irrelevant whether we use small letters or none at all (as Castle prefers) for the allelomorphs. If they are a part of the Mendelian machinery, who cares very much what we call them?

If then we have here two pairs of allelomorphs, crossing over may take place, as it does in other cases where two pairs of linked

[1] THE AMERICAN NATURALIST, June, 1914, p. 383.

genes are involved.[2] This is all there is to the matter. We need not dwell, therefore, at length on Professor Castle's statement that here is another case of an erroneous conclusion reached in consequence of using small letters for "absent" characters, except to remark that Dexter did not use small letters for absent characters, and that the erroneous conclusion has been drawn by Professor Castle himself.

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PROFESSOR MORGAN has called my attention to the fact that in criticizing a single point in Mr. Dexter's review I have given the impression, to some at least, that I regarded Dexter's views as erroneous. Such was not my intention, and I wish to correct the impression, if I may. I do not for a moment question the reality of "unit-character" inheritance or indorse the idea of "the organism as a whole" as the only inheritance unit. I agree here entirely with the view which I understand Dexter to hold. If Nabours has encountered nothing but simple allelomorphs among his grasshoppers (which I neither assert nor deny), this by no means proves that only simple allelomorphs exist even among said grasshoppers. An organism which seems to have only one variable "gene" may nevertheless possess any number of other genes which are not varying so far as we can discover, and in which consequently all zygotes are homozygous and all gametes similar to each other.

It is only in Dexter's discussion of the significance of the exceptional "*B E I*" individual that I should dissent from any part of his excellent review. Nabours's explanation of this case, according to Dexter, is essentially that of "non-disjunction," instead of which Dexter himself offers the explanation of "linkage," and proposes a repetition of the experiment to decide between them. Now I do not question for a moment the genuineness of either "non-disjunction" or "linkage," as they occur for example in *Drosophila*. Through the kindness of Professor Morgan I have been able to demonstrate both these phenomena repeatedly to classes in genetics in the course of their laboratory work upon *Drosophila*. The point which I wished to make in com-

[2] Crossing over would not take place if the factors in question were allelomorphic. If the case is one of non-disjunction the subsequent generation would also give a different kind of result from that of linkage. (See Bridges, *Jour. Exp. Zool.*, 1913.)

menting on Dexter's review (and this is the *only* point in which I dissent from his opinions) is that the repetition of the experiment, provided it had the outcome suggested by Dexter, would leave us as much in the dark as we were before concerning the correct interpretation of the result. Very likely, however, *additional* facts might be observed which *would* give some clue, so that I quite agree with Dexter's suggestion that the case should receive further study. But I can not see that *at present* linkage has more in its favor as an interpretation than non-disjunction.

The "demonstration" which Mr. Dexter gave of his argument by introducing duplicate "symbols" instead of the single set used by Nabours, seemed to me quite superfluous and possibly to have been a real stumbling block in the logical process. This is why I raised the question as to the significance of the small letters. The terminology is that of the "presence-absence" hypothesis, as commonly understood, but Professor Morgan assures me that such is not the significance which Dexter attaches to the symbols used. It seems to me therefore that the significance attached to the symbols is vital to the argument in the "demonstration."

I quite agree with Professor Morgan, however, that symbols are a matter of small consequence. Suppose we omit the "demonstration" by means of symbols altogether. Should we then have any reason to favor linkage as an interpretation rather than non-disjunction? I can not see that we should have. It seems to me quite possible that neither explanation will prove adequate.

When albino mammals are crossed with colored ones, piebalds sometimes are obtained in later generations. So far as we know, these result neither from "non-disjunction" nor from "crossovers." Perhaps the *B E I* individual also is a *tertium quid.*

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